Art. XIII.—*On Finding the Longitude from Lunar Distances.*

By E. J. White, F.R.A.S.

[Read Thursday, October 10, 1889.]

About a fortnight ago the newspapers contained an account of the sufferings of the crew of the ship Garston, which had been wrecked, owing it is stated to a fault of the chronometer. On reading this, I was led to reflect on the methods at present in use for finding the position of a ship at sea. The latitude is obtained in so simple and so direct a manner that no notice will be taken of this operation, beyond remarking that the principal desideratum is a night instrument, to allow of altitudes of the stars being observed at other times than twilight, at which time only can the ordinary sextant be relied on for this purpose. The longitude problem is more complex, it resolves itself into two questions—finding the local time, and that of the first meridian, which to English speaking people, and indeed to most others now, is that of Greenwich. The observation for finding the local time is as simple as that for latitude, but the calculation is a little more complex, and it is dependent on the latitude, less so, however, when the body observed is east or west than in any other position. The finding of the Greenwich time is a more serious matter; it exercised the minds of men for many centuries, and large rewards were offered by the British Parliament to stimulate investigation. As far as navigation is concerned, the methods have been reduced to two—timekeepers, and moon observations, generally known as chronometers, and lunar distances. The chronometer method is so simple that it has now nearly superseded the other (I was informed by a naval officer a little time ago that he had never seen a lunar distance taken for finding the place of the ship, but only for the instruction of the naval cadets); but in my opinion, it is criminal to rely upon it for long voyages when less than three chronometers are carried, for such delicate machines are always capricious, and even the slight rusting of the balance spring would render it quite untrustworthy. The observation and reduction of lunar distances has been a favourite pastime with me for forty years, and from the experience thus obtained, I have formed such a high estimate of the method, that I have viewed with sorrow its gradual disuse. Many reasons have been given for this state of affairs—one is, that seamen have not time for the necessary calculations, but with Thomson's tables, which are quite accurate enough for navigation, the time required for a reduction is only ten minutes. Another, and this I consider the true reason, is that the results could not be trusted. In the consideration of this it may be stated that about thirty years ago the lunar tables were so inaccurate that an error of twenty-two miles of longitude would sometimes be due to the Nautical Almanac alone. In 1862, Hansen's tables were first used in this work; these gave the place of the moon very accurately for some years, but commenced to diverge a little time ago, to counteract which, the Nautical Almanac applies Newcomb's empirical correction to the places of the moon used in computing the lunar distances, and the outstanding error is now very small indeed. Besides the errors of the Nautical Almanac, there are those of the sextant, which owing to excessive competition and lowering of price, are larger than formerly. It is quite surprising how rarely one meets with a really good sextant nowadays. Those made by the celebrated Troughton at the beginning of this century are still unsurpassed. I took the following excerpt from the catalogue of a manufacturer who exhibited at our last exhibition:—"We guarantee any of our best sextants to have a small margin of error (under 3′, and generally under 1′ of arc)." As the index error is easily and always ascertained by the observer, I suppose the error referred to is independent of this, principally excentric and division error, which is so troublesome to measure that it is generally neglected by seamen; and how would these errors affect a longitude determined from a lunar distance? by about thirty times their amounts—90′ and 70′ of arc respectively. I once had brought under my notice a modern sextant, where the excentric error amounted to 9′. If such a one had been used in a lunar distance, the error of longitude would have been about four degrees and a half. In other cases, sextants originally very good, have been ruined by incompetent repairers, or bungling treatment by their owners. Those who wish to test the lunar method should practise at a place whose longitude is well known. The index error should be carefully found at each observation. The same dark glass or glasses should be always used for the sun, the grading of the brightness being obtained by the up and down motion of the telescope, and the contacts should be alternately made on and off. The errors of the resulting longitudes can then be tabulated or graphically described, whence a table of corrections for the different parts of the arc can be made. Some observatories contain special apparatus for testing sextants, but an ordinary observer would have more confidence in the former method. To those who wish to study the general theory of the sextant, I would recommend "Simms on the Sextant and its Applications," published by Troughton and Simms, of London, as the best work on the subject in our language. It is not, however, an easy book to read, and the notation is cumbrous and uninviting. For an example of finding the errors of a sextant from astronomical observations, the best work is the "Treatise on Practical Astronomy, as applied to Geology and Navigation," by Professor Doolittle, of the Lehigh University. The work was carried out by Professor Boss, the present director of the Dudley Observatory, and the various steps of the process are given with the minutest detail. The greatest correction found was 38 seconds of arc, which shows that the instrument was a very good one, for Simms states in the work before referred to, that there are few sextants in which the error which varies with the reading does not amount at its maximum to 40 seconds of arc. In many it exceeds 1 minute, and instances are to be met with where it amounts to 5 minutes.

For reducing a lunar observation, or clearing the distance as it is technically called, many methods have been devised, indeed, no other astronomical problem has shown such a fecundity of results, but their principles may be broadly divided into two—one the absolute solution of the two astronomical triangles presented by the problem, the other the differential variations of the parts of these triangles. The former of these is now seldom used, owing to the amount of work and care necessary; it is, however, the only safe one to use when the observed distance is very small. The shape in which this computation is carried out, is generally some slight modification of Borda's formula, published near the end of the last century. The large tables of Mendoza Rios are also adapted to this method. At sea, the reductions are generally made on the differential principle, mostly by the aid of Thomson's tables, which give in a convenient shape, the result of an immense amount of labour, as the author stated that he solved more than 80,000 lunar distances in their construction ; but for hydrographic and land observations, they are not sufficiently precise. In these cases, the rigid formula was, until lately, generally applied. About thirty years ago, however, Chauvenet, in his great work on Special and Practical Astronomy, gave a new investigation and tables for its application, by means of which the whole of the corrections can be taken into account, and the result obtained with nearly the accuracy of the laborious method of Bessel, who, as Chauvenet remarks, is the only one who has given a theoretically exact solution of the problem.

From the many hundreds of lunar observations I have taken in Australia, I select my observations of the last three years, as I wish to show the degree of dependence on them in the present state of the Nautical Almanac. They were all observed at my quarters, a little to the south-west of the Observatory. The instrument was a pillar sextant, Troughton No. 1139, it was made at the beginning of this century, and has been in my possession nearly forty years, during the whole of which time the index error, which is measured at each observation, has barely varied half a minute of arc, and the greatest excentric error is about twenty seconds. The objects observed, have been in every instance the sun and moon, which, from their slower relative motion, are theoretically less suitable than the moon and a star; but I find the results to be actually better, owing to the delicate contacts that can be made with two discs, and the advantage of daylight in noting the time and reading off the arc. Only one coloured glass has been used in all the observations, the equality of brightness of the two images having been obtained by altering the distance of the telescope from the plane of the instrument. No special selection of time has been made for observing, indeed from the fact that I keep my sextant at home, most of the observations have been made before 8 a.m. or after 5 p.m., when one of the objects is usually rather low, whereas on board ship the most favourable times could always be chosen. The barometer and thermometer have been read immediately after each observation for the proper correction of the refractions, and the reductions have been made by means of Chauvenet's tables. The corrections for excentricity have not been applied; the results, therefore, are those given by a good sextant used in the ordinary way, with careful reduction. The local time has been taken from a chronometer watch, compared with the mean time clock of the Observatory. On a few occasions, it was taken from a clock beating seconds, whose error was determined in the same manner, and in only one instance is there a doubt about this element. The clock was used in the observation of October 14, 1888—it will be noted as the most discrepant but one of the series—it was reduced soon after it was observed and the discrepancy remarked, and on examining the clock, I found that in that part of the dial where the minutes were read, the zeros of the second and minute hands did not correspond, and I have reason to believe that a mistake of a minute was made in the local time; as I have, however, a great dislike to cooking observations, I have not changed the original entry of the observation. As the altitudes are in every case computed, a mistake in the mean time would affect them also.

Date.	Approximate observed Distance.	☾	Resulting Longitude.	Errors true obs.	Remarks.
1887.					
d. h.	° ′		h. m. s.	sec.	
Aug. 27, 5	107 52	E	9 40 49	— 55	Good.
Sept. 10, 21	85 55	W	39 43	+ 11	Cloudy.
Oct. 8, 19	105 3	W	39 24	+ 30	Very good.
Nov. 19, 23	62 43	E	40 29	— 35	Good.
Dec. 6, 19	105 40	W	40 8	— 14	Cloudy.
,, 8, 19	79 48	W	39 52	+ 2	Good.
,, 9, 19	66 28	W	40 9	— 15	Good.
,, 10, 19	53 5	W	39 48	+ 6	Good.
1888.					
Jan. 21, 7	91 22	E	39 23	+ 31	Cloudy.
,, 22, 3	101 19	E	39 58	— 4	Good.
,, 24, 6	124 17	E	40 19	— 25	Good.
Feb. 4, 20	88 9	W	39 45	+ 9	Good.
,, 15, 6	37 21	E	39 44	+ 10	Moon faint.
,, 17, 6	59 28	E	39 18	+ 36	Good.
,, 18, 4	69 59	E	38 58	— 4	Good.
,, 20, 6	92 29	E	39 55	— 1	Sun clouded.
,, 21, 6	103 34	E	40 28	— 34	Good.
June 2, 23	72 39	W	39 55	— 1	Cloudy.
Sept. 29, 20	71 52	W	39 12	+ 42	Good.
Oct. 14, 4	115 26	E	40 58	— 64	Good. Minute of local time doubtful.
Dec. 22, 19	130 5	W	39 16	+ 38	Good.
,, 24, 19	106 39	W	39 33	+ 21	Very good.
,, 27, 19	68 8	W	40 20	— 26	Good.

Date.	Approximate observed Distance.	☾	Resulting Longitude.	Errors true obs.	Remarks.
1889.					
Jan. 11, 6	115 15	E	40 45	— 51	Moon faint.
,, 21, 19	125 9	W	40 26	— 32	Very good.
,, 22, 19	112 28	W	39 55	— 1	Very good.
,, 25, 19	73 19	W	40 22	— 28	Cloudy. Moon faint.
,, 25, 22	72 19	W	40 16	— 22	Moon faint.
,, 26, 20	59 31	W	40 37	— 43	Good.
,, 27, 19	46 3	W	41 30	— 96	Cloudy. Very unsatisfactory.
Feb. 19, 21	129 38	W	39 49	+ 5	Hazy. Moon faint.
,, 20, 21	116 44	W	39 59	— 5	Good.
,, 22, 21	90 24	W	40 3	— 9	Very good.
Mar. 6, 5	51 50	E	38 46	+ 8	Moon faint.
,, 7, 6	63 6	E	39 40	+ 14	Sun clouded.
,, 10, 5	95 35	E	39 55	— 1	Very good.
,, 11, 5	106 43	E	40 32	— 38	Very good.
,, 12, 5	117 47	E	40 36	— 42	Good.
,, 13, 5	129 8	E	40 9	— 15	Good.
Aug. 3, 3	72 51	E	39 17	+ 37	Good.
,, 4, 1	84 28	E	39 58	— 4	Good.
Oct. 2, 5	92 49	E	39 17	+ 37	Very good.

The mean of the whole 42 observations is 9 h. 39 m. 59 s., which only exceeds the adopted longitude, as determined by telegraph, by 4·8 s. The mean of the 21 with the moon east is 9 h. 39 m. 59 s., of the remaining 21, with the moon west, is 9 h. 40 m. The probable error of a single observation is found from the residual errors to be ± 21 s., or a little more than 5 miles of longitude.

The result is, in my opinion, very satisfactory, for much larger errors than these have been found in the longitudes of places which have been determined with the large instruments of astronomical establishments. In one instance, that of the Lisbon Observatory, an error of nearly nine seconds of time was discovered a few years ago, when the longitude was measured by means of the electric telegraph. As observations can be taken at sea with nearly the same ease as on shore, except perhaps in the case of steamers, where the smoke and vibrations are very annoying, the results should be equally good, if the same care is taken in the reduction ; but where Thomson's and similar tables are used, the probable error would be rather larger.

In conclusion, I may express the hope that this paper may contribute towards rescuing this once favourite method of finding the longitude from the neglect into which it has undeservedly fallen, and that it may lead to a stricter supervision of the instruments employed in nautical astronomy. I believe that all gun barrels have to be submitted to a Government test, but if one burst, little harm would be done, except to the user. In the case of sextants and chronometers no supervision is exercised in their manufacture, and the selection is left to individual caprice, yet a faulty one may cause the loss of much property and many lives.

Art. XIV.—*On the Pseudogastrula Stage in the Development of Calcareous Sponges.*

By Arthur Dendy, M. Sc., F.L.S.

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(With Plate 1a.)

[Read November 14, 1889.]

Thanks to the researches principally of Metschnikoff, Schulze and Barrois, we are now in possession of a tolerably full and accurate account of the development of the Sycon type of calcareous sponges, as represented by the genus *Sycandra*. It is in the hope of contributing a small addition to our knowledge in this department of embryology that the present paper is written.

Before going on to describe my own observations, it will be advisable to give a brief account of the now generally accepted views concerning the history of the development of *Sycandra*—such, for example, as is to be found in Balfour's "Treatise on Comparative Embryology."

The ovum is a naked, amœboid, nucleated mass of protoplasm, which, after fertilization, undergoes the early stages of its development within the tissues of the mother sponge. The ovum first divides vertically into two and then into four segments. The next two divisions are also